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## Social Psychology

### A gloss on attribution theory

Gün R. Semin

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This paper consists of a critical examination of specific assumptions made by attribution theory about everyday epistemology. The first and main stipulation discussed concerns the model of man contained in attribution theory which is derived from a model of a statistician or a raw data processor. It is argued that this model poses considerable difficulty in explaining how everyday life is interpreted and understood, since it disregards the social context in which interpretation and understanding takes place. Further, it is argued that the model imputes assumptions of rationality to everyday life similar to those of a rational statistician. With this assumption attribution theory verges on ideology in its interpretation of everyday explanations of events. Finally, two further themes are addressed. The former, the twofold cause classification of explanation into situational and dispositional, is critically assessed. The latter concerns a serious shortcoming of attribution theory, namely its lack in specifying the conditions under which attributions are made in everyday life and those under which no attribution is made. In the concluding section the implications of these criticisms are then discussed.

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The highly influential writings of Fritz Heider (e.g. 1958) have led to the formulation of models which are concerned with the psychological 'premises' underlying everyday epistemology, and indeed the seminal works of Jones & Davis (1965) and H. Kelley (e.g. 1967, 1972*a, b*, 1973) have laid the necessary groundwork for what is probably the most influential framework in today's social psychology: attribution theory. The presently burgeoning area certainly contains the promise which most social psychological theories have not had, namely the integration of hitherto disparate fields under a common theoretical umbrella (cf. Harvey & Smith, 1977).

However heretical it may seem in view of attribution theory's current hegemony, the argument advanced in this paper is that the model of man in attribution theory carries certain implications which need to be surmounted if the theory is to be *psychologically* informative about the 'processes' through which explanations are achieved in everyday life.

The critical arguments are divided into four parts, as follows. The central question concerns the status of the model of man employed in attribution theory. With the advent of attribution theory the 'naive psychologist' has acquired '... a status equal to that of the scientist who investigates him' (Ross, 1977, p. 174). Hence, it is deemed to be '... not inappropriate for attribution theorists to use the formal procedural rules of science as a model for Everyman's inferential processes' (Harvey & Smith, 1977, p. 14). This, it is suggested, disregards the role played by the complex heritage of centuries of preinterpreted experiences distilled by human beings and constantly and systematically handed down to their offspring. This preinterpreted experience, generally subsumed under the name of culture or custom, provides us in our everyday life with intersubjective guidelines to interpret events, and thus provides the social context of our existence (cf. Semin & Manstead, 1979), a context which is disregarded by attribution theory. In the second part of this paper the status of attribution theory is discussed and it is argued that apart from the specific model of man attribution theory also contains a prescriptive stance regarding the evaluation of self-knowledge and knowledge of others which comes dangerously close to an ideology. The next issue which is addressed concerns a central theme in attribution theory, namely the twofold cause classification into situational (external) *versus* dispositional (internal) causation. This 'dichotomy' has already elicited some critical

comment from attribution theorists themselves (e.g. Kruglanski, 1975; Ross, 1977). The argument presented here is that despite its potential heuristic value this classification does not afford a fuller understanding of the social context within which attributions are made and all too often remains an ambiguous interpretation leading to prescriptive statements. Finally, the discussion will centre on the conditions under which we do and do not make attributions in everyday life, that is the conditions under which we invest *attributional effort*.

### Critique of the model of man as statistician

If one starts with George Kelly's extrapolation of 'man as scientist' (Kelly, 1955, chs 1-3) and his view that behaviour can generally be seen in terms of man's attempts to understand, predict and control his environment, then the question is: how does he do it? Indeed this is precisely the question attribution theorists have asked themselves (e.g. Heider, 1958; Jones & Davis, 1965; Kelley, 1967, 1972*a, b*, 1973). In answering this question, however, attribution theorists have inadvertently projected a statistician into their model of man and, further, both central attribution theories have regarded the 'scientific' interpretation of 'cause-effect relationships' as being the criterion against which all other interpretations of cause-effect relationships should be evaluated.

The model of the statistician is clearly central to Kelley's writings: 'It is convenient to conceptualize the process under discussion, the inference of cause from the observation of covariance, in terms of the analysis of variance employed in psychology to interpret experimental results' (1973, p. 109). If the 'naive psychologist' has a single datum instead of data he is assumed to employ a 'causal schema'. 'More specifically (and this is the link between the analysis of variance and the schema ideals), the causal schema can be viewed as "an *assumed pattern of data* in a complete analysis of variance framework". It is within this assumed configuration of data that the single observation is fitted and interpreted. "Given information about a certain effect and two or more possible causes, the individual tends to assimilate it to a specific assumed analysis of variance pattern, and from that to make a causal attribution" (Kelley, 1972, p. 2)' (quoted from Kelley, 1973, p. 115, emphasis in the original).

However, the meaningful conduct of everyday life is based on the possession of common, known-to-all rule systems with multifarious reference (i.e. to specific events, behaviours at such events, human nature, the ego and the alter), as well as rules which enable the interlocking of such reference points. The following hypothetical story illustrates the point. A Brazilian aborigine visits Rio by invitation of the Ministry of Aboriginal Affairs. On returning to his tribe he gives his friends an account of this bizarre religious orgy to which he was taken. 'On particular days more people than all those you have seen in your whole lifetime roam to this huge place of worship, an open hut the size of which you will never imagine. They come, chanting, singing, with symbols of their Gods and once everybody is gathered the chanting drives away all alien spirits. Then, at the appointed time the priests arrive wearing colourful garments, and the chanting rises to war cries until three high priests, wearing black, arrive. All priests who were running around with sacred round objects leave them and at the order of the high priests begin the religious ceremony. Then, when the chief high priest gives a shrill sound from himself they all run after the single sacred round object that is left, only to kick it away when they get hold of it. Whenever the sacred object goes through one of the two doors and hits the sacred net the religious followers start to chant, piercing the heavens, and most of the priests embark on a most ecstatic orgy until the chief high priest blows the whistle on them.' If our aborigines wished to understand this social reality and the causes of this odd behaviour, to make sense of it they would at least have to know the rules of soccer: no analysis of variance is going to help them, however scientifically or naively applied. Furthermore, the attribution theorist

will need to know the social reality of the aborigine if he wishes to understand the aborigines' attributions.

The conduct of our everyday life presupposes distinct rule structures with their distinctive types of 'languages' (cf. Peters, 1969) for a variety of settings and behaviours in such settings. The social in social behaviour refers to a collection of 'conventions' created and adopted by a collectivity and as such is objectivated and anonymous (Semin & Manstead, 1979). This common stock of knowledge at hand enables the individuals belonging to the collectivity to jointly construct and maintain a mutual social reality. From this viewpoint it follows that the rule-following, social and cultural dimensions of everyday social existence are central in the pursuit of the question of how we meaningfully interpret our social reality and engage in meaningful social action and interaction. Because if the aim is to understand how social explanation takes place, then the examination of the nature and organization of this common stock of knowledge which is culture-bound and historical (Schutz, 1953; Elias, 1977) is inevitable.

However, it is precisely these rules, conventions, and metaphors, central as they are to our everyday existence, that attribution theory neglects in imputing a statistical model of man and regarding the interpretation of everyday reality as one which follows statistical rules into which 'raw data' need to be fed.

A further related point arises from the assumption made in attribution theory that the criterion by which everyday interpretations of social reality must be 'evaluated' is constituted in the 'scientific' interpretation of 'cause-effect relationships' (cf. Jones & Davis, 1975, p. 220; Kelley, 1973, p. 109). Through this criterion attribution theory has instituted the foundations of what Ross (1977) aptly described as '...two distinct but complementary goals. One goal has been the demonstration that, by and large, social perceivers follow the dictates of logical or rational models in assessing causes, making inferences about actors and situations, and forming expectations and predictions. The other goal has been the illustration and explication of the sources of imperfection, bias, or error that distort such judgements' (1977, p. 179). This has indeed been an inspirational source (cf. Nisbett & Wilson, 1977; Nisbett & Ross, in press; Ross, 1977, 1978).

The implication is that the most rational attribution of causality is that proposed by scientific research. The particular definition of the domain for research, i.e. how do we make causal attributions, is unproblematic, since we generally take for granted the 'fact' that we make causal attributions and often we cannot explicitly state how we have been able to do so. However, what does become problematic is the taken-for-grantedness in attribution theory that the 'naive psychologist's' phenomenology contains a conception of rationality that is similar to, if not identical with, the 'rationality' of analysis of variance. The next section deals with the elaboration and implications of this problem.

### **The prescriptive element in attribution theory**

If 'science' is regarded as interchangeable with 'analysis of variance' and 'rationality' is regarded as synonymous with 'science', then the inescapable conclusion is that such a view can only be prescriptive, and means that the theories in question are normative, containing prescriptions as to how people 'should' understand themselves and others. By following the three tricks which Smith (1974)\* regards as the 'recipe' for making ideological representations of what people think (as will be illustrated later), it can be shown that attribution theory, particularly as developed by Kelley, achieves precisely this. The ambiguity of the twofold cause classification, as we shall see later on, is precisely the loophole which enables the attribution theorist to achieve an 'intuitive leap', namely fitting what people say into what attribution theory suggests people to mean.

\* I am grateful to Andrew Durling who drew my attention to Smith's (1974) work.

In contrast to Jones & Davis's (1965) model, Kelley's model is more broadly conceived, encompassing not only personal dispositional attributions, but also self and situational attributions. Further, it attempts to be more precise in specifying how the attribution process continues should no personal attribution be made. Jones & Davis (1965) do not provide any specifications on this issue, although they do imply that other actions altogether have to be considered as well as the action observed. In effect, Kelley suggests that, in order to solve the uncertainty that enters Jones & Davis' model when the attribution of personal disposition cannot be confirmed, the actor's environment has to be subdivided into 'entity', 'time/modality' and 'persons', so that the three criteria of distinctiveness, consistency, and consensus can be applied in determining to which particular aspect of the environment the action can be attributed. These three criteria are the constituents of the 'covariation principle', which states that 'an effect is attributed to the one of its possible causes with which, over time, it covaries' (1972, p. 3). This principle applies when the attributor has multiple observations available. Thus, if there is covariation along only one dimension, then the action will be attributed to that aspect of the environment containing covariation; *or* the action may be attributed to a personal disposition, the precise attribution depending on how the three dimensions are defined by the attributor. In the case where only a single datum is available, the 'discounting principle' is applied by the attributor, namely: 'The role of a given cause in producing a given effect is discounted if other plausible causes are also present' (Kelley, 1973, p. 113). However, as quoted earlier (p. 292) Kelley regards both covariation and configuration principles as related, inasmuch as an analysis of variance pattern is striven for in both instances.

The application of these principles is precisely the point at which there is vagueness in Kelley's model, since it does not specify how an attributor identifies which part of the environment is 'entity', 'situation', or 'persons', or how an attributor knows to which dimensions the criteria of distinctiveness, consistency, and consensus should be applied. In fact, the implicit if not explicit assumption would appear to be that the model proposes an individual operating in a 'social vacuum' (Tajfel, 1972, with Moscovici's 1973 proviso), processing something which resembles 'raw data'. This impression is strengthened in Ross's writings (cf. 1977, p. 174), where socially preinterpreted input is regarded as 'raw data' and its validity is judged by statistical principles. This leaves the attribution theorist with an awesome burden, in that he has to impute the social content and meaning in each instance himself, constantly plugging the gaps of the model with 'intuitive leaps' from his implicit assumptions about the nature of social reality.

The main issue here is that the 'fundamental enterprise' (Jones, 1977) which is singled out by attribution theorists and which is regarded as a fundamental enterprise of 'the man of the street' is 'finding the causes for behaviour' (Jones, 1977, p. 317). The weak line is that the attribution theorist takes as his point of departure end-products of explanation in everyday life and imputes processes of a supposedly psychological nature to explain how these products are achieved.

In an earlier paper I have argued that such inference is generally impossible and that the sources which can potentially account for the regularities in the dependent-independent variable relationship are manifold as long as the distinction between the social and the psychological properties of social behaviour are not clearly made (Semin & Manstead, 1979). The social properties of social behaviour are regarded as those rules, conventions and norms that are potent guides for behaviour regularities. The psychological properties of social behaviour are regarded as those processes which enable the identification, elicitation and monitoring of such social codes.

Since both the attribution theorist and his subjects share the same language and cultural

background they both have access to the same shared knowledge about the rules that govern the relationship between specific 'stimuli' and their 'responses'. However, as pointed out in an earlier section, if the social knowledge within which behaviour is made meaningful is ignored, namely the rules and conventions which are crucial in the meaningful interpretations of social reality, then the ground is prepared for the first trick in Smith's (1974) recipe for making an ideological representation of what people think, namely: 'Separate what people say they think from the actual circumstances in which it is said, from *the actual empirical conditions of their lives and from the actual individuals who said it*' (p. 41, emphasis added).

The next step consists of regarding attribution theory, particularly with the imputed ANOVA model, in precisely the way suggested by Kelley himself: 'If I repeatedly refer to attribution *theory*, it should not lead the reader to expect too much in the way of a systematized set of assumptions, propositions and deductions. "Theory" is used here in a broad and, I would insist, *entirely appropriate sense*, to refer to a more or less *plausible set of general principles offered to explain certain observed phenomena*' (Kelley, 1973, p. 108, emphasis added). This, however, corresponds to Harré's (1974) summary of the neo-positivistic stance, namely: 'The neo-positivistic idea of science was that the compilation of a catalogue of laws describing such regularities completely exhausted the content of a science' (p. 241). The aim of such a theory is to bring order into data, and it is never clear whether or not such a theory corresponds to a real state of the mind, i.e. whether the suggested processes do in fact have any phenomenological reality.

Thus, with the postulation of some analysis of variance model the second 'trick' for Smith's recipe is obtained, namely: 'Having detached the ideas, they must now be arranged. *Prove then an order among them which accounts for what is observed*' (1974, p. 41, emphasis added).

The third 'trick' suggested by Smith's (1974) recipe is obtained as follows. The foregoing implies that the crucial value judgement that attribution theorists adhere to when re-attributing their models to 'reality' is this: 'consistency is... a *criterion* for understanding, a criterion for when the causal explanation is thought to be sufficient' (Jones *et al.*, 1972, p. xi). To explain, attribution theorists make the crucial assumption that each individual constructs his causal attributions in a systematic manner, but because they take the possibility of causes and effects for granted (i.e. unquestioningly accept that 'causes' and 'effects' are readily identifiable, discrete entities in everyday life), this systematic nature of causal attributions is not considered to be possibly indicative of a cultural rule-system guiding the perception of actions and events. Rather, the systematization is assumed to be a part of each individual's cognition, and the attribution theorists' models clearly claim to represent this cognition. Thus, every attribution, if it is to achieve 'true understanding', must *consistently* follow the models, and it is at this point that the models reveal a prescriptive, rather than explanatory nature, for it follows that if any attribution 'deviates' from this consistency, it must be deemed 'irrational': this is a 'value-judgement'.

This prescription is implicit in Shaver (1975), for in considering the 'category mistake' he states that 'attribution is a *psychological* process that may or may not be entirely rational, and it is clear that perceivers do attribute some behaviour to enduring personal dispositions presumed to exist apart from the overt actions that can be observed' (p. 70, emphasis in original). Now, it may be true that, according to formal logic, the ascription of causality to a 'personal disposition' is a mistake, but although Shaver (1975) recognizes that both Jones & Davis (1965), and Kelley (1967) make the same mistake in their theorizing, it is not valid for him to conclude that, because attributors do not perceive their mistake, this must be due to a universal *cognitive* process. Indeed, it could be said that

attribution theorists exhibit one of their own 'irrational attributions' – namely, 'too little account is taken of external causes (contextual factors) in judgements of other persons' behaviours' (Kelley, 1972, p. 18) – when they claim that their models are representations of cognitive processes. That is, they attribute agency to their models even though these models may just be representative of the external cultural context within which these models were constructed, a context in which 'cause-effect' terminology is taken for granted as a part of everyday life. And this precisely constitutes the third ingredient for Smith's (1974) recipe. 'The ideas are then changed "into a person", that is, they are constituted as distinct entities to which agency (or possible cause efficacy) may be attributed. And they may be re-attributed to "reality" by attributing them to actors who now represent the ideas' (p. 41).

Thus, Shaver makes the same mistake as all other attribution theorists in assuming that the very ability to know when, where, and how to use the very notion of a 'cause' is unproblematic, for 'suspending our certainty' in having that ability is precisely the standpoint of 'radical doubt' that constitutes the starting-point of a truly phenomenological analysis of causal attribution. Such an analysis would therefore not set out to decide whether or not an attribution is 'right' or 'wrong' (i.e. whether or not it is consistent with an actual state of affairs to which it is supposed to correspond), but rather would seek to describe what people do to make the concept of 'causality' something *that can be said*. When such an analysis is done, then it will have reflexively explained how it is that it was able to theorize about causal attribution in the first place.

#### **The twofold cause classification**

One of the major loopholes through which the dubious translation between what is said and what is meant is obtained, is contained in the broad twofold cause classification, i.e. the attribution of cause either to an enduring, stable property (dispositional) or to an environmental contingency (situational). The problematic nature of this cause classification should already be evident from the preceding discussion. The twofold classification is crucial for attribution theoretical inquiry at all three general points of its dependent variable assessments, namely assignment of cause, social inference, and prediction of some future act or outcome from some observed event (cf. Jones *et al.*, 1972; Monson & Snyder, 1977; Ross, 1977). Together these three tasks constitute the locus of ambiguity, as well as constituting the origin of an 'intuitive leap' by the attribution theorist.

The problem lies primarily in the 'subjective interpretation of meaning' (Weber, 1947) and the neglect of this in attribution theory was argued in the preceding section, namely the ambiguity that is afforded by *post hoc* explanations of attributions with a disregard for the social context. The difficulty lies not only in the point that this twofold classification *is not* a dichotomy (cf. Monson & Snyder, 1977, p. 90; Ross, 1977). As Ross (1977) points out, attribution theorists very often ask the subject to explain why a particular act occurred, and then the answer is coded into 'situational' vs. 'dispositional' accounts. The ambiguity is brought out in his example of 'Jack bought the house because it was so secluded'. This can be coded according to any criteria and regarded as either dispositional (he likes seclusion) or situational (the house was secluded), and the problem is not avoided in answers which are closed-ended, since the same interpretational ambiguity will prevail. As Monson & Snyder (1977) have noted, we are nevertheless recommended to retain this classification, albeit with some provisos; namely, as long as attributions 'imply or state' (p. 90) one or the other explanation. Despite the possibility that such classification may have some merits we are left with no clear criteria on the basis of which to classify subjects' answers, and are left with the impression that Kruglanski's more radical criticism is the only one to adopt under such circumstances, namely that '... the use of the external-

internal distinction in reference to attributional findings has typically lacked explicit rationale. Rather it seems to have been grounded in an arbitrary semantic decision to characterize some purposes in a "motive" language (that has an "internal" feel to it), and other purposes in a "goal" or object language (that feels "external") (1975, p. 390).

### **The notion of 'attributional effort'**

Attribution theory's disregard for the social context and the inevitably prescriptive nature of the theory gives rise to a further serious shortcoming, namely it makes the man in the street a continuous 'reality constructor'. It is obvious that we do not construct social reality from scratch every morning when we get up; however, it is not clear in attribution theory *when* we make attributions and *when* we do *not*. If it were the case that we had to explain the causes of behaviour constantly, the cognitive load would be intolerable, and we would have our man literally lost in thought. As argued in the first section, our everyday life consists of rules, conventions and guidelines, both for behaviour and its interpretations. In the flow of everyday life we do not constantly ask the questions 'why?'. Only when there is a temporary 'breakdown' of the rules, only when social reality has slipped temporarily through our fingers, do we need to invest some effort in reconstructing the reality. As Mills (1940) pointed out: '...Men live in immediate acts of experience and their attentions are directed outside themselves until acts are in some way frustrated. It is then that awareness of self and of motive occur. The "question" is a lingual index of such conditions. The avowal and imputation of motives are features of such conversations as arise in "question" situations' (p. 905). However, since attribution theory does not make any statements about the nature of social reality, or the social context within which actions are designed and acts executed, it also does not contain any specifications as to when we ask ourselves the question 'why?'. That we ask the question 'why?' is taken for granted, but its aetiology is not examined at all. The least one has to do is to specify those minimal conditions under which the question 'why?' is asked (as Mills does), that is identify the conditions under which attributional effort is invested.

Generally rule systems themselves contain explanations or accounts for the people acting 'in rule', such that interlocutors have their socially defined 'invariant motives' (cf. Schütz, 1953; Semin & Manstead, 1979). These rule systems enable the invariant attribution of motives, and posing the question 'why?' in such contexts becomes somewhat tautological (e.g. 'Why is he delivering the mail?' – 'Because he is the postman'). Only when there is a 'violation' of the unquestioningly taken-for-granted reality of everyday life does the 'why?' question become relevant, and only then is attributional effort invested. However, in order to specify this question, the theory must contain some recognition of, or statement about, the content of social reality. If such statements are not forthcoming, as is the case in attribution theory, then one is left with the rather unfortunate position of having to explain every single occurrence in everyday life, as if it were an unprecedented act. What is more, if the content of everyday life, namely the rules which govern the running of everyday life, are not clearly identified with their properties, then one runs the danger of *imputing 'processes' to regularities which are rule governed*, or alternatively *deriving 'processes' from regularities which are historical*.

The 'why?' question is a question about reflection, i.e. regarding one's self or one's acts, or for that matter the alter and its acts, retrospectively as a detached observer. That is also why the agent in attribution theory has not really achieved its required status of 'emancipation', for it is never able to reflect properly and its answers will always be those which are of a raw data processor and not of a 'scientist' in G. Kelly's sense.



### Conclusions

Attribution theory's chief shortcoming, according to the present argument, is its disregard for the social context of everyday existence, and this shortcoming is manifest in the model of man implicit in the theory. However, 'any scientific understanding of human action, at whatever level of ordering or generality, must begin with and be built upon an understanding of everyday life of the members performing those actions' (Douglas, 1971, p. 11). The use of formal ('scientific') categories defined in advance of their examination (cf. Douglas, 1971, p. 13) leads in the case of attribution theory to a clearly prescriptive position which is difficult to distinguish from 'ideology'. Finally, the disregard of social context leads to the lack of any distinctions between the conditions under which attributions are made and those under which they are not made.

The difficulty of presenting unambiguous empirical research for attribution theory is inherent in the rather loose specifications advanced by the theory. Since the processes which lead to an attribution of cause are not in themselves the object of inquiry, but a matter of conjecture, and since the inferences about such potential processes take place after the assignment of cause is made, there is ample room for speculation. This is illustrated for instance in the studies concerned with the so-called reverse placebo effect. Despite the fact that conflicting sets of data are obtained, the results are explicable within an attribution theory framework (e.g. Storms & Nisbett, 1970; Kellogg & Baron, 1975; cf. Bootzin *et al.*, 1977). Further, since the original theory does not specify how an individual actually applies the criteria of distinctiveness, consistency and consensus to entities, persons and situations, there is further room for speculative conjecture. Finally, the inherent ambiguity of the twofold cause classification enables one to fit any data pattern within the 'theoretical frame'.

Recent work within an attribution theoretical framework suggests that a conceptual revision of the cause classification is required (e.g. Zimmermann, 1978*a, b*). The emerging critique points out that one has to take into account 'reasons' for behaviour as well. Buss, in his eloquent critique (1978), argues that whereas 'causes' can be considered as a category which refers primarily to 'occurrences', namely behaviour that happens to a person, 'reasons', as a logically distinct category, refers to behaviour done by a person, 'an action'. What is important is that whereas earlier research (McArthur, 1972, 1976; Orvis *et al.*, 1975; Ruble & Feldman, 1976) supportive of Kelley's criteria of external-internal attribution, when analysed in terms of 'occurrences' and 'actions' is not supportive of the criteria any more. Broadly speaking, actions require a different attributional logic than assumed in the model (cf. Zimmermann, 1978*a, b*).

The major problem, however, resides in the lack of a heuristically fruitful and an analytically consistent distinction between content and process with respect to social behaviour. That there is still considerable difficulty in drawing such a distinction is apparent in the illuminating critique that Smith & Miller (1978, pp. 359-361) have applied to the internally conflicting distinctions drawn by Nisbett & Wilson (1977).

Basically, there are a number of different ways in which one could approach the content-process distinction in social psychology (e.g. Semin & Rogers, 1973; Semin & Manstead, 1979) and indeed, whatever distinction one wishes to impose will remain one that is arbitrary, since the object in social psychology does not antedate the perspective. Far from it. It is the viewpoint that creates the object and there are no criteria which tell us in advance that one perspective is superior to the other in any particular way. However, what remains crucial in our view is the recognition of the *socio-logic* of everyday life, which is worthy of much more systematic study than has been afforded in social psychology so far (cf. Harré & Secord, 1972, for an exception) and the *psycho-logic* of the processes which

enable the employment and deployment of such 'systems', i.e. rules, conventions, etc. How these two logics, one derived, the other given, interact and coexist remains the task of a psychologically informative social psychology.

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